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Dear Phil:

I have some interesting news, but first have to beg another favor of you in order to be able to continue to develop the problem, namely we have just about run out of the single factor V(or should I say Salmonella O-5) serum that you sent me last year. We tried a shot at making this ourselves, but although the antigen (an alcoholized 4,5,12 TM) agglutinated quite well, the resulting serums (2 rabbits) had practically only 4,12 antibody. Are there any tricks? Meanwhile, are you able to let us have some more of the V serum, or a substantial volume of a mixed 4,5,12 with a high 5 component?

This goes back to that abortus-equi 4,5,12"trandduction"that you remarked upon some time ago, and that I was reluctant to accept as a transduction of 5. I should have noticed this before, but just a few weeks ago, I remembered a pair of TM cultures that Bruce and I had worked on, the SL-15 and SL-18 of the Stocker-Zinder-Lederberg paper in last year's J.G.M. (see table3). The curious thing about this otherwise identical pair of O-variants was that one was 4,12, non-lysogenic for phage; the other was 4,5,12 lysogenic for phage A2. (Both also carried B2). In relation to the Iseki-Uetake results then, I suggested to Aleck Bernstein that he try to convert the 4,12 to a 4,5,12 by infecting it with A2 and other phages.

The experiment has worked, in a manner of speaking, several times, and similar results were obtained with abortus-equi also. But the role of the phage is not yet so clearcut. This isn't transduction in a typical sense, for the frequency of the antigenic change is about a hundred-thousand times greater, and the effect (in one experiment) does not depend on the serotype of the cells on which the phage had been grown. On the other hand, the correlation between lysogenization and acquisition of 5 is not perfect, though very high. But I think we can clear up some of these questions fairly soon if we can replenish our supply of the critical reagent. I would also like to send you some of the converted cultures for confirmation of serotype (under separate cover in a few days).

A Professor Uetake, at Sapporo, Japan, had been in correspondence with me about his results, rather similar to Iseki's but apparently independent of them, and I suggested he submit his work to the Journal of Bacteriology. The enclosed copy of his ms. draft needs some revision, but I thought you would be interested to borrow it for a time. By the way, Lou Baron (Washington) told me he had successfully confirmed the principal observations. You probably will also have seen Iseki's report of typical transduction (low frequency and host-dependent) with the same group E phage he uses for the lysogenic conversion (high frequency and host-independent).

When will we have a chance to talk things over again? Sincerely,